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Discussions

Interactive comment on "Distributed model of hydrological and sediment transport processes in large river basins in Southeast Asia" *by* S. Zuliziana et al.

Anonymous Referee #1

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The manuscript "Distributed model of hydrological and sediment transport processes in large river basins in Southeast Asia" by Zuliziana et al. presents a water and sediment model and its application to two large river basins in SE Asia. Building upon an existing hydrological model, its coupling to a model for suspended sediment is described in detail. The model also contains a simple module for considering reservoirs. The model is applied in daily resolution in two case studies. The authors evaluate the model performance by comparing measured and modelled monthly fluxes of water and sediment. The authors' stated objective was "to develop a process-based distributed model that can simulate the sediment dynamic process at a large basin scale". I have

several doubt that a) this is a significant scientific objective to merit publication in HESS (i.e. methodological paper) b) this objective was thoroughly achieved (i.e. case study paper).

a) The author's review of existing sediment models is inadequate. Apart from (R)USLE and SWAT the complete research in sediment modelling is completely ignored. Properly identifying a niche or gap of knowledge for the proposed model is essential to justify its scientific novelty. Since the proposed model is completely built on well-known approaches, I cannot see this novelty here.

b) The authors advocate for a process-based model because of their spatially distributed concept and high temporal resolution (p. 3, ll. 17) . The confusion between a model's conception (i.e. physical vs. empirical) and its spatial structure (i.e. lumped vs. distributed) notwithstanding, the study addresses or verifies neither spatially explicit model output nor data in high temporal resolution. An advantage of a physically-oriented (I prefer that term) model that the authors do not mention may be transferability. Still, in the current case lab-scale equations are applied to 100-m-cells or even 1 km resolution, which makes the correct representation of physics (and thus parameter transferability) questionable. This is also evident in the need for the model parameters to be calibrated. The model evaluation is not very rigorous: Using observed dam outflow as operation rule makes modelling discharge somewhat trivial for the downstream gauges. Nearly half of the catchment is controlled by these reservoirs in the Thailand example. Validating with monthly data for river flow in a monsoon-dominated catchment easily leads to seemingly good NS-values (easily predictable seasonality of great magnitude, consider e.g. benchmark Nash-Sutcliffe coefficient NSEbench, Schaefli and Gupta, 2007). The SSC-skill seems to be nil (negative NSE); 10-year SSY with underestimation by factor more than three is not impressive. The discussion of the model shortcomings is limited and quite speculative. Other shortcomings of the model (insufficient raingauge network, river overboarding, unaccounted reservoirs,...) probably deserve more discussion.

Further issues: - Description of hydrological and sedimentological model lacks clarity and comprises numerous inaccuracies, but is still very lengthy - numerous imprecise formulations that leave the reader with many questions. - reservoir siltation is altogether neglected in the Mekong case - including it is imperative when modelling sediment at this scale - Using r as a measure of fit is misleading at best - The performed sensitivity analysis is very crude an restricted to few variations of even fewer parameters. The conclusions drawn from it do not seem well-founded to me. - The authors claim the model be "useful for management and stakeholders". I wonder what information does the model give that cannot be obtained from the available measurements? - Especially when publishing in an open access journal, Model availability should be explained; ideally, the code should be provided.

More remarks are contained in the annotated PDF.

I have skipped chapter 3.2 (the Mekong case) in my review, because the abovementioned issues alone do not allow me to recommend the publication of the manuscript. For this, a) the scientific of the model needs to be shown (pointing out the new models advancements) and b) the model testing needs to be much more rigorous (e.g. higher temporal resolution, application to ungauged catchment). I think this would be another study beyond the scope of major revision. I recommend to reject the manuscript.

Please also note the supplement to this comment:
http://www.hydrol-earth-syst-sci-discuss.net/12/C3182/2015/hessd-12-C3182-2015-supplement.pdf

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 6755, 2015.